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DO KITTENS INSTINCTIVELY KILL MICE?[1]

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The observations which we here report supplement in certain respects the results obtained by Berry[2] in this laboratory, and suggest the need of alterations in his conclusions concerning the reactions of kittens to mice. Upon his study of the behavior of a litter of Manx kittens Berry based the following statements, which we wish the reader to consider in connection with the facts which it is the purpose of this paper to present. "I am also led to believe that cats are credited with more instincts than they really possess. It is commonly reported that they have an instinctive liking for mice, and that mice have an instinctive fear of cats. It is supposed that the odor of a mouse will arouse a cat and that the odor of a cat will frighten a mouse. My experiments tend to show that this belief is not in harmony with the facts. When cats over five months old were taken into the room where mice were kept they did not show the least sign of excitement. A cat would even allow a mouse to perch upon its back, without attempting to injure it. Nor did the mice show any fear of the cats. I have seen a mouse smell of the nose of a cat without showing any sign of fear."

"It was not until the mouse began to run that the interest of the cat was aroused. The cat then ran after it, playfully striking it with her paw, becoming rougher the longer she played with it. The instinct seems to be for the cat to run after anything which runs from it. I think it is evident that it is through imitation that the average cat learns to kill and eat mice. If this is true, it shows the extreme importance of imitation in the mental development of the cat. Further-

[1] From the Psychological Laboratory of Harvard University.

[2] Berry, C. S., 'An Experimental Study of Imitation in Cats,' *Jour. of Comp. Neurol. and Psychol.*, Vol. 18, pp. 1–25, 1908.

more, it indicates that much that has commonly been attributed to instinct is, in reality, due to imitation" (pp. 24–25).

Believing that the experiments upon which the above generalizations are based should be repeated with younger kittens than those observed by Berry, we have systematically studied the reactions of eight kittens when in the presence of mice.

The kittens belonged to two litters. Of these, the first, consisting of four kittens, was born in the laboratory on March 2, 1910, of a black and white cat which had been obtained through an animal dealer in Boston a few days previously. The second litter, also of four kittens, was born on May 11, 1910, of a maltese and white cat which had been furnished by the same dealer two weeks previously. Hereafter we shall refer to the several kittens by number as follows:

First Litter Born March 2, 1910.		Second Litter Born May 11, 1910.	
No. 1	Male	No. 5	Female
No. 2	Male	No. 6	Male
No. 3	Female	No. 7	Male
No. 4	Female	No. 8	Male

The cats, and later the kittens also, were kept in the animal-room of the laboratory, with the freedom of the room much of the time. No other animals were kept in the room, and special precautions were taken to make the room mouse-proof, so that the observers might obtain complete histories of the kittens' experiences with mice. The diet of the cats consisted of fresh milk, beef (usually cooked), and fish. The kittens were given fresh milk as soon as they would accept it, which was during the fourth week. All of the individuals were in perfect health throughout the experiments and exhibited normal development, but the members of the first litter were better nourished, more active, energetic, and aggressive than those of the second.

In reporting our results we shall describe first the experiments with the eight kittens previous to the opening of their eyes.

Each individual was tested, in the presence of both observers, at least twice during the first week of life, with the odor of mouse. This was done by bringing a live mouse near to the nose of the kitten. In no instance was a reaction observed which differed essentially from the reactions to such odors as weak ammonia, sour yeast, leather, and the experimenter's hand. The animals gave no evidence of special interest in the odor. Usually there occurred twitching of the nose; rarely opening of the mouth; and still more rarely, something between hissing and spitting.

From these observations, which we report thus briefly because of limited space, we conclude that during the period of post-natal blindness kittens tend to avoid the unfamiliar odor of mouse, just as they avoid other strange odors, and, further, that the odor has no special significance with which a definite instinctive reaction is correlated.

In stating the results of our tests with the animals after they had gained their sight, we shall deal with the litters separately.

These tests consisted in placing each kitten separately in a cage with wire mesh sides and cover, 45 cm. by 30 cm. by an average of 20 cm. deep, in which a mouse had been placed. Here the kitten was kept under continuous observation for fifteen minutes. The eyes of the members of the first litter, nos. 1–4, were fully opened on March 14, and on that date the animals were tested, at the age of twelve days. They exhibited no reactions to the mouse, beyond looking at it momentarily as it happened to approach or touch them. Neither the olfactory nor the visual stimuli from the mouse called forth anything which resembled an instinctive reaction. In general it may be said that the kittens paid no other attention to the mouse than to a bit of dry bread in the cage.

On April 1, when slightly over four weeks old, nos. 1–4 were again tested. The first three reacted to the mouse only by following it now and then with their eyes as it happened within the field of vision. They neither chased nor made attempts to touch it. The behavior of no. 4 contrasted markedly with that of the others. She noticed the mouse, soon after she had been placed in the cage, as it moved near her, and quickly seized it in her mouth, growling the while. The mouse escaped and the kitten gave chase, but failed to recapture it before it had climbed to the top of the cage. In this position the kitten seemed unable to recognize it, and she made no further attempts to find it during the fifteen minute interval.

In the behavior of no. 4, a female, at the age of four weeks, we thus have unmistakable evidence of the presence of the killing instinct. Had the mouse been smaller, or had the cage rendered it more easily accessible, it certainly would have been killed. It was evident that the kitten's stage of visual development did not enable it to follow the rapidly moving mouse or to recognize it at a distance of twenty to thirty centimeters.

Five days later, on April 6, the kittens of the first litter were tested for the third time.

No. 1 approached the mouse and smelled of it, but he drew back quickly when his nose touched it. He seemed to be startled, mewed

loudly, and tried to escape from the cage. Later he several times approached the mouse and smelled of it as at first. There was no evidence of a killing instinct. We have repeatedly seen the kittens react similarly to inanimate objects.

No. 2 smelled of the mouse, when the latter's movements had attracted its attention. It also followed it about the cage with its eyes for brief periods. Interest soon failed and the kitten mewed persistently to be let out of the cage.

No. 3 at first tried to escape from the cage. Upon seeing the mouse move she trembled noticeably, as if in fear. Later, when the mouse happened to touch her, the kitten spit and hissed vigorously.

No. 4 smelled about the cage as though searching for something. She finally saw the mouse in one corner of the cage and made for it, but she was too slow to catch it, for the mouse had quickly darted to the top of the cage as the kitten began to move. The kitten continued to the place where she had seen the mouse an instant before, and after feeling and smelling in the corner where the mouse had been she licked her paw. Soon she located the mouse at the top of the cage. She approached, reached up with one paw and touched the mouse, then drew back. This was repeated three times. Had the mouse run from its position, the kitten doubtless would have attempted to catch it. After a few minutes the kitten again noticed the mouse, this time nibbling at a bit of dry bread on the floor of the cage. She immediately approached it, and as it ran she pursued and tried to catch it. As the time for the test had elapsed the kitten was now removed to the animal-room.

On April 8, the fourth test was given kittens nos. 1–4.

No. 1 showed more interest in the mouse, by following it readily and persistently with the eyes and moving about after it. When he happened to touch it, he spit and struck at it with his paw playfully (?). The mouse was not afraid of the kitten and did not run away.[1]

No. 2 attended immediately to the mouse and began to play with it. After a time he accidentally sat down on the mouse, whereupon it squeaked. This aroused the kitten and caused it to strike with its paw and lick its lips. It then followed the mouse about the cage at intervals.

[1] These experiments have revealed marked differences in the behavior of mice toward kittens. We have used wild gray mice, and laboratory gray, black, and agouti mice. Under the conditions of our experiments, the wild mice always exhibited extreme fear, whereas of the laboratory mice some were obviously fearful and others fearless of the kittens. Whether the differences are due to experience or to inheritance we have not attempted to determine.

No. 3 behaved as in the previous test. When the mouse came near her, she would spit and move away.

No. 4 exhibited characteristically interesting behavior. On being placed in the cage, with back arched she pursued the fleeing mouse to a corner and then spit and struck at it with her paws. Next she bent her head low and sniffed at it. She watched it closely for a time, showing sustained attention. When the mouse finally attempted to escape from the corner, the kitten made a dash for it, hissing and growling. On overtaking her prey, she drew back slightly, spitting and repeatedly unsheathing and sheathing the claws of a paw which she extended toward the mouse. The mouse next made a sudden dash across the cage. Like a flash the kitten was upon it mouth and claws. With the mouse held firmly by the head, the kitten began to growl. In a few seconds she shifted her hold and taking the nose of the mouse she bit it repeatedly. Then followed a period of worrying during which the mouse several times tried to escape only to be seized by some part of its body and vigorously chewed or struck with the paws of its captor. Within fifteen minutes of its capture the mouse had been bitten and worried to death. The kitten immediately began to chew at one of its legs, which it finally succeeded in eating, but the skin of the mouse was so tough that the kitten soon abandoned its attempt to eat its prey.

At this point, in order to determine the effect of delaying for several weeks the development of the ability of a kitten to kill mice, tests with nos. 1 and 3 were discontinued and only nos. 2 and 4 were used.

On April 14 the latter individuals were given their fifth test. No. 2 smelled of the mouse and drew back. He followed it momentarily with his eyes, but soon lost interest and paid no further attention to it. No. 4 chased the mouse, as in the previous test, spitting, striking with exposed claws and otherwise exhibiting excitement. Owing, however, to the ability of the mouse to keep out of reach most of the time, the kitten did not succeed in capturing it within fifteen minutes.

April 20 was the date of the sixth test. No. 2 watched, followed, and struck at the mouse repeatedly. He showed more sustained attention than previously. No. 4 behaved as in the foregoing test. She did not succeed in capturing the mouse.

On April 22 the tests were made in the mouse-room of the laboratory instead of in the experiment room which had been used for the other tests. No. 2 exhibited no new reactions. He did not follow the mouse persistently. No. 4 seized the mouse immediately by the head, growling as she chewed at it. She had eaten it completely within six minutes.

Again on April 25 no. 2 was tested in the mouse-room. From the first he followed the mouse and struck at it with his paw. Soon he succeeded in cornering it and seizing it by the neck and back. Growling he chewed vigorously at its head and back until it was dead. Within five minutes he had eaten his prey. In this test the kitten's interest in the mouse increased greatly when the latter showed fight.

Finally, on April 27 both no. 2 and no. 4 were again tested in the mouse-room. No. 2 at first attended to the mouse playfully, but suddenly as the mouse ran across the floor of the cage this playfulness was replaced by the killing instinct. The kitten gave chase, seized the mouse, and by biting it through the head killed it immediately. When another kitten was brought near the cage no. 2 growled warningly.

No. 4 immediately began to sniff about the cage. As soon as she caught sight of the mouse she ran and seized it. The mouse managed to escape, whereupon the kitten excitedly gave chase and again caught it. This time she bit its head so severely that it was helpless. Later the mouse was killed and eaten.

On May 16 a different sort of test was given. Each kitten was set free in the mouse-room with a mouse. No. 2 was interested in the mouse, but he showed only slight ability to keep track of it among the furnishings of the room (cages, table, radiator, boxes, etc.). Nevertheless, he had discovered it, by reason of its movements, within a few minutes, and he immediately captured it. Subsequently, after playing with the mouse for several minutes — repeatedly allowing it to escape and then recapturing it, tossing it into the air, clawing, and striking it with its paws, etc.— the kitten killed its prey by biting it about the head, and ate it. No. 4 also had difficulty in keeping track of the mouse. She spent much time smelling about the room as if in search of it. Finally she saw the mouse move. Capture followed. The captive was then played with for a while, after which it was killed by being bitten through the head and neck, and eaten.

This concludes our brief description of the behavior of nos. 2 and 4. We have shown that these two kittens — the female within a month after birth and the male in less than two months — without opportunity for imitation and without other experience with mice than that described above, began to kill and eat mice. We feel fully justified, in view of the characteristics of the behavior, in calling it instinctive.

With the thought that the instinctive tendency to kill mice might wane or become increasingly difficult to develop as kittens aged, we postponed tests with nos. 1 and 3 until May 26. These kittens were

therefore almost three months old when we resumed our tests with them.[1]

On May 26 nos. 1 and 3 were given their fifth test, the fourth having been given on April 8. This test was given in the mouse-room. No. 1 appeared to be aroused by the odor of mouse in the room and ran about when brought into the room, as if searching for something. This observation, which we made in connection with several of the kittens, would seem to indicate that the odor of mouse may become significant to the kitten even before it has made its first kill. When placed in the experiment box with a large gray mouse, the kitten smelled of it several times, followed it about the cage, and finally struck at it with his paw, immediately thereafter springing back as the mouse moved. Nothing further of interest happened during the interval of experimentation.

No. 3 smelled about, located the mouse, and watched it intently. She hissed as it moved. Subsequently she followed the mouse closely and struck at it with claws exposed. Once she brushed it from the top of the cage with a paw. As it ran to one corner of the cage, she followed and struck it several times with her paw: her tail switched and her ears twitched a great deal during this activity. During the remainder of the interval, she clawed at the tail of the mouse as it cowered in a corner of the cage.

Again on May 28 the kittens were tested in the mouse-room. No. 1 was attentive to the mouse from the first. He watched and followed it, growling the while. It was evident that the movement of the mouse attracted him. At last the mouse attempted to run across the cage. The kitten made a sudden dash for it, seized it by the back, and at once killed it by biting through the spine in several places. He then switched his tail, growled, and hissed as he stood over his prey. Repeatedly he picked up the body, sometimes tossing it into the air, and each time striking it with his paw as it fell to the floor. The mouse was not eaten.

No. 3 licked her lips when placed in the cage, and her ears twitched noticeably as she crouched and watched the mouse. Occasionally she spit. As the mouse ran to the top of the cage she followed and struck it. With back arched she watched it intently and steadily for a few seconds, then she brushed it down with a paw. This happened a number of times. At the end of the interval the kitten was closely watching and following the mouse.

[1] It is to be remembered that the kittens studied by Berry were at least five months old when he first tested them with mice.

The eighth test was given to nos. 1 and 3 on June 1. No. 1 caught the mouse as soon as he saw it. He growled as he bit through the back of the mouse and killed it. About half of the body was eaten at once. No. 3 behaved as in the previous test. She did not attempt to kill the mouse.

On June 3 only no. 3 was tested. She smelled about the cage, and when she discovered the mouse at the top she crouched. She then stealthily approached and struck with exposed claws. As the mouse retreated she followed. In less than a minute she had seized it and so severely bitten its neck that it was helpless. The kitten growled, hissed, and otherwise exhibited excitement the while.

The results of these tests with two kittens three months old, when considered in connection with those of Berry on kittens five months old, suggest that the killing instinct (of the existence of which we are convinced by our observations) tends to become increasingly difficult to evoke as the animals age. This in part, doubtless, accounts for the fact that some cats will not kill mice at all, whereas other are more or less eager to do so.

We are inclined to believe that Berry's results would have led him to quite another conclusion (he definitely states "that cats do not instinctively kill and eat mice, but learn to do so by imitation," p. 25) had he tested his Manx kittens at the age of one to two months in a cage which did not permit the mice readily to get out of sight or reach of the kittens.

The behavior of the members of the second litter, Nos. 5–8, will be described only with respect to certain points in which it differed from that of the kittens of the first litter.

As previously stated, the reactions of Nos. 5–8 while yet blind furnished us with no indications of an instinctive adjustment to the odor of mouse. After the opening of their eyes, these individuals were systematically tested on May 26 and 29, and on June 1 and 3. In none of these tests did anything noteworthy which has not already been recorded appear.

Beginning with the test of June 7, when the kittens were about four weeks old, we may describe the behavior of the several individuals. For this test small gray mice about ten days old were used.[1] Their eyes were not yet open and they of course could not run from the kittens. They crawled about the cage considerably during the experiments.

No. 5 sniffed when placed in the cage, in the mouse-room. Her attention was taken by the movements of the mouse and she looked at

[1] In all previous tests well grown mice had been used.

it for several seconds. Although she did not touch it with her paws at first, she several times placed her nose close to it, and finally licked it gently with her tongue. Next she took it in her mouth and bit it hard enough to make the mouse squeak, but not sufficiently to break the skin. This was repeated three times. Then the kitten left the mouse. She gave no signs of excitement or of an instinct to kill. Rather her behavior was indicative of mild interest and playfulness.

No. 6 looked at the mouse momentarily as it crawled about and touched it with his nose, but he paid no further attention to it.

No. 7 was attracted by the movements of the mouse and touched it with his nose. He then left it. After twelve minutes he happened to be so placed that he could see the mouse as it began to move. For a few seconds he watched as if fascinated by the sight. Then he moved directly and quickly, in spite of shakiness of his legs, to the mouse, and seized it in his mouth by the middle of the back, at the same time biting hard, and bending his head to the floor so that one paw could be placed firmly on the body of the mouse. In a few seconds, he had bitten the mouse to death. Without pause the process of eating was begun. It proved a difficult task, notwithstanding the tenderness of the mouse, but after ten minutes of diligent effort and much gagging it was completed.

Here we have a kitten one month old, still so feeble and unpracticed that he tottered as he attempted to walk, seizing, placing his paw upon, and eating a small mouse after the manner of cats. Anything more striking in the way of an exhibition of the killing instinct would be difficult to imagine. The observation proves that given conditions which favor its appearance—namely, a mouse suited in size and strength to the size of the kitten and a cage which does not permit it to get beyond the kitten's field of vision — the killing instinct may appear even during the first month of life. Although meat had been fed to the mother, it is extremely improbable that this kitten had ever before tasted meat of any sort, and it is certain that he never before had tasted mouse flesh. Indeed, he had only a few days before the test begun to lap milk from the food dish.

No. 8 merely looked at the crawling object for a second or two and then lay down for a nap.

On June 16 all of the kittens were again tested. No new reactions appeared. Not one of the individuals except No. 7 made any attempt to follow or capture a mouse. He caught, killed, and ate his mouse in a calm business-like way.

The members of the second litter were further tested on June 26,

27 and 28. On none of these dates did any of them kill a mouse. On the twenty-seventh no. 5 struck and bit one so badly that it lay as if dead, but she paid no further attention to it after once seizing and then dropping it.

After the twenty-eighth of June the experiments had to be discontinued, notwithstanding the fact that the records for the members of the second litter were incomplete.

Rather dogmatically stated, and with meager discussion, our conclusions are as follows:

1. Kittens possess the instinct to kill mice. We are not prepared to say that the reaction to mice differs essentially from that given to other small living things, but we have clearly demonstrated by a variety of tests that the reaction to a mouse differs radically from that given to lifeless objects which are moved before the animal.

2. The instinct to kill may manifest itself in the kitten before the end of the first month of life, while the animal is yet feeble and barely able to eat a young mouse. It more commonly appears during the second month.

3. The instinct appears suddenly. In a moment the playful kitten becomes transformed into a beast of prey. The picture of the play instinct differs as greatly from that of the killing instinct as does the picture of joy from that of rage.

4. The reaction is fairly definite in character, complex, and highly adaptive. It involves the bodily states of attention; muscular tension; bristling of the hair; sometimes erection and sometimes switching of the tail; hissing and, at times, spitting; growling; unsheathing and sheathing of the claws; use of the mouth, teeth, paws, and claws. The picture varies somewhat with individuals.

5. The instinctive reaction is aroused (*a*) by the movement of the mouse (if the animal keeps quiet it is not noticed by the inexperienced kitten); (*b*) by the odor of the mouse (this seems to be relatively unimportant for the first reaction; subsequently it is of great importance). Berry is quite right in saying that the kitten has an instinct to chase any small object that runs from it. If the object when overtaken behaves as does a mouse, the killing reaction is likely to appear. We should say, then, that the visual factor of movement is the primary condition for the initiation of the killing instinct; that the odor of a mouse becomes markedly significant with the first kill and subsequently plays a part; and that the visual impression of the form of a mouse becomes important as the visual ability of the kitten develops. It is almost certain that cats usually recognize mice by their odor and movements and not by their form or markings.

6. The instinct does not completely wane during the first three to five months of a kitten's life, but it apparently becomes increasingly difficult to evoke. The practical inference is: allow a kitten to exercise its killing instinct when young if a good mouser is desired.

7. Although opportunity neither for imitation nor for experience with mice is necessary for the efficient execution of the killing reaction by kittens, there can be no doubt that each of these conditions ordinarily contributes to the awakening of the killing instinct. Cats bring dead or injured mice, or other small animals, to their kittens. Thus, early in life, the animals become familiar with the odors of their natural prey. Undoubtedly, then, the average kitten would react to a mouse during the second month of life more quickly than did those observed in the laboratory.

The whole point of our work, it is to be noted, is the study of the instinct to kill as it is exhibited by kittens which have been deprived of everything in the nature of preparation for dealing with mice.

8. We deem as chiefly important in our observations, the fact that kittens, even in their first kill, so seize the mouse that they cannot be bitten by it. In almost every instance our kittens caught their mice by the head, neck, or back in such a way that the animals were helpless. We present this fact as a point in favor of the specificity of the reaction to mice. An untutored observer certainly would have inferred from the behavior of these kittens that they had learned just how to seize mice in order to prevent them from biting.

9. The killing instinct with surprising rapidity lends itself as the basis for the acquisition of habits of dealing with mice. Racial and individual experience soon become so completely intermingled in the behavior of a kitten toward mice that only in the light of a minute and thorough knowledge of the latter can the instinctive features of the behavior be identified. Whereas at first the kitten tends to kill immediately upon capturing a mouse, it thereafter tends rather to delay the fatal bite. At first the reaction is performed in a business-like way; later the kitten plays with its prey for minutes at a time without seriously injuring it. Again, the beginner attempts to capture a mouse only when it runs, whereas the experienced kitten begins to search for its prey as soon as it sees the box in which it once killed a mouse, or as soon as it detects the odor of mouse.

10. It is our impression, although statistically our results do not justify a statement of fact, that in the female kitten the instinct to kill is more highly developed than in the male.

PSYCHOLOGICAL LITERATURE.

ANIMAL PSYCHOLOGY.

Vorlesungen über Tierpsychologie. KARL CAMILLO SCHNEIDER. Leipzig: Engelmann, 1909. Pp. xii + 310.

The author of this book is professor of zoölogy at Vienna. He has published widely upon theoretical questions in biology. The present volume is based on lectures delivered last summer (1909).

Professor Schneider approaches the problems of animal psychology from the point of view of the biologist. His interest lies in explanation, not description. He is an outspoken teleologist. He stands directly opposed to Bethe, Uexküll and other 'mechanistic' physiologists. The book is obviously designed as a counterblast to the 'nurphysiologische' psychology which seeks to eliminate the 'psyche.' For the author, consciousness, or more precisely the 'psychical,' is literally universal. There exists, then, an unlimited panpsychic reservoir for supplying and expanding the animal mind. The fundamental form of consciousness, in the concrete, is the telic idea (*Zweck-* or *Ziel-* or *Finalvorstellung*). In the simplest creatures this process is nothing less than an innate idea which derives directly from the *Allgemeinbewusstsein* or universal mind. Telic ideas or *finalia* are prior to and independent of experience. They rule the protozoan and invertebrate mind, they are subject to modification in the vertebrates, and in man they are suppressed by reason, personality, and individual experience. They are forms of psychical energy essential to the functional unity of the body. They operate through the impulse (*Trieb*), which is the mainspring of animal action. Primitive action is analyzed, early in the Lectures, into sensation, feeling, need (*Bedürfnis*), impulse, and telic idea. But later on feeling proper is denied to all animals below man, and sensation appears only as the realization of the telic idea. Moreover, the distinction between need and idea-of-end seems to be purely logical; *e. g.*, amœba is hungry and knows (by a prophetic insight) that it needs nourishment. The idea of nourishment — to expound further the primitive action — is attended by a release of psychical energy and is followed by appropriate movements of capture. This is the primal form of impulsive action. The main factors in it are idea and *Trieb*. It is found in the simplest

animal organisms. On the basis of it Schneider proceeds up the scale, developing, as he goes, more than twenty other forms of action. All forms, however, are reducible to four main types: impulse (lower invertebrates), instinct (higher invertebrates), initiation, or trial-and-error (vertebrates), and reason (man). Although these forms are not wholly confined to the groups named, they represent, nevertheless, successive stages in the development of consciousness. For the two higher forms, the world of stimulus furnishes a large part of the material. Man especially works out and builds up an 'inner' world from the 'outer' world of experience. Impulsive action, on the other hand, is chiefly determined by innate ideas, and innate ideas absolutely dominate the instinct. The author's preoccupation with teleological principles suggests a mental interpretation even for automatic and reflex movements. Both are 'psychical.' In the former, consciousness is faded and suppressed, but still present; in the true reflex, it appears as an isolated 'segmental psyche.'

Schneider uses four different 'criteria' for mind. Three of these rest upon its integrative functions. Physiological processes summate only. But animal actions cannot be explained as the result of simple summation. They rest upon 'agglomeration.' Response to an object as such (plurality of stimuli) implies 'apprehension'; *i. e.*, a perceptual synthesis or association of qualities, the formation of a unitary whole, not a mere amalgamation of physiological processes. The integration of perceptual, memorial, and 'rational' processes now furnishes the first criterion; that of the 'central subject'—which the author thinks is necessarily implied in behavior—the second; and the integration of over-individual or social impulses, the fourth. The adaptive character of action furnishes the third criterion. Animal behavior is essentially purposeful, and nothing but the 'psychical,' as in the form of the telic idea, can truly be said to be purposeful. "All these criteria raise the existence of consciousness above the possibility of a doubt and compel us to take account of mind as we take account of known material substances, forces, and energies." Schneider's psychophysics rests upon current physiological hypotheses. His chief debt is to his adversaries, Bethe and Uexküll. He posits four mental functions (sensation, memory, telic idea, and *Trieb*) and four neural functions (excitation, tension, conduction, and regulation). (Notice the preference for four-fold classifications!) He does not hesitate to assume the existence of sense-cells and memory-cells, of final-cells and Trieb-cells. His psyche emerges passively in the receptor centers and discharges finally, as active psyche (*Trieb*), in the effector

apparatus that controls muscular tonus (*Tonuserzeuger*). It is *Trieb* — not the nerve-cells — that maintains the functional unity of the nervous and muscular systems. Mind is then ultimately responsible, not for its own substantial existence alone, but also for the vital government of the body.

The psychologist's estimation of Professor Schneider's acute and thoughtful Lectures must rest upon their usefulness to the student of the animal mind. By way of information, the book contributes little. Its tone is controversial and constructive. It attempts a new interpretation; an interpretation that rests upon ontological speculation. Instead of appealing to the human mind and to the human body, it appeals to Platonic ideas, Leibnitzian monads, and a Hartmannian Unconscious. It may be questioned whether such an appeal is either necessary or fruitful. The author thinks that it is necessary first because speculative biology fails to explain the evolution of mind, and secondly because physiologists have, in his opinion, done violence to the organism by rejecting a causal mental factor. Had he been a psychologist, he would have realized that an explanation of the animal mind without concrete description and analysis is a waste of time, and that for description and analysis speculative biology and metaphysical construction are equally bad substitutes. He would have seen further that a *nurphysiologische* physiology is quite consistent with a *nurpsychologische* psychology, provided only that each science knows and attends to its own business. As to the productiveness of Schneider's method, it must be conceded that so long as one is content to introduce *ad libitum* a teleological principle one will never be in want for explanations. See how the method works. Once assume the innate idea and the innate idea immediately becomes five — idea of need, movement, position, form, place, according to the problem to be solved ; once assume an energetic psyche and the energetic psyche grows under the hand into a subject, which divides conveniently into central and peripheral, passive and active, individual and social subjects. Small wonder that the author gets on without 'intelligence' until he comes to man. With Aladdin's Lamp one doesn't need a standing army! As to the concrete problems of comparative psychology, the reviewer is uncertain whether the more pressing and difficult of them — *e. g.*, imitation, instinct, the modes of learning, orientation, the psychology of the social insects — are materially advanced toward solution.

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LITERATURE OF THE PAST YEAR ON THE BEHAVIOR OF LOWER ORGANISMS.

1. Tropisms.

At the sixth International Congress of Psychology held in Geneva, 1909, four prominent investigators, Bohn, Loeb, Jennings, and Darwin, were asked to discuss the subject of tropisms. The first three named responded, but one of these, Jennings, did not appear at the Congress in person. The ideas of Bohn and Loeb regarding the nature of tropisms are essentially the same; but while the former devotes his entire address to the definition of the term, the latter has for his principal theme the significance of tropisms to psychology. According to both of these authors there are three fundamentally different reactions in organisms. One (tropism) is dependent upon the continuation of a constant external condition. It is the function of the intensity (i). The second (*Unterschiedsempfindlichkeit*, *sensibilité différentielle*) is dependent upon the time rate of change in the external condition. It is a function of di/dt. The third is dependent upon associative memory, and associative memory, Bohn says (p. 8), is the function of "(a, b, c, $\cdots$)." What this means, I do not know.

Bohn[1] discusses the characteristics of tropisms under seven different heads.

1. *Symmetry.* — "A tropism," he says, "is the consequence of a quantitative difference between the activity of the two halves of a symmetrical body due to asymmetrical stimulation." Under this he gives two criteria of tropisms: (a) If an organism which is exposed to a stimulating agent from two or more sources goes toward a point between them, the reaction is a tropism; (b) if an organism with the sense organs on one side of the body destroyed makes circus movements, *i. e.*, turns continuously toward either side, when the stimulus is symmetrically applied, the reaction is a tropism.

2. *Stimulation.* — As already stated, tropisms are considered by the author to be a function of the constant intensity. Both sides of a body are continuously stimulated proportionally to the absolute intensity of the stimulating agent on the two sides. When the organism is oriented, the stimulation on the two sides is equal, and both sides move at the same rate. When it is not oriented, the absolute intensity of the stimulating agent on the two sides differs, and consequently the

[1] Bohn, G., 1909, 'Les tropismes,' *Rapport au VIme Congrès International de Psychologie*, Genève, 15 pp.

rate of movement of the two sides differs until the intensity on the two sides becomes equal.

3. *Variability.* — "It is not possible," says Bohn, "to conceive of an invariable tropism," because the chemical state of living matter varies.

4. *Irresistibility.* — All activities of animals, including tropisms, are strictly determined by physico-chemical processes.

5. *Experience.* — The tropisms are independent of experience.

6. *Adaptation.*—Tropisms are not fundamentally adaptive. They often conduct inferior animals into regions which are fatal.

7. *Complexity.* — Tropisms are due either to difference in the rate of locomotion or to growth on opposite sides of the body. They may consequently be as complex as the processes underlying locomotion or growth.

This definition of tropism is in essential agreement with the ideas of Loeb on the subject. As a matter of fact practically all of the points made by Bohn can be found in Loeb's earlier papers, and in the address in question Loeb emphasizes these points again as essential characteristics of the tropisms; but, as already stated, his main object in this address is to show the bearing of these reactions on the analysis of psychic processes. The address[1] is divided into eleven sections, the contents of which are summarized below.

I. The analysis of all psychic processes must be grounded on physico-chemical laws. It must begin in the lower forms and proceed to the higher. What is termed 'will' in many lower animals is nothing more than the phenomenon of tropisms.

II. The reactions of aphids to light are cited to illustrate tropisms. If these creatures are exposed to light from one source they orient and move toward the light quite directly. "Two factors determine the locomotion of the animals under these conditions; the first is the symmetrical structure of the animal and the second the photo-chemical action of the light." If the light on the two retinas is of unequal intensity, then the chemical reactions in the one which receives more light is stronger than in the other. This causes the muscles on the two sides of the head to contract unequally, which forces the head to turn so as to face the light, and the rest of the body must of course follow. If one eye is destroyed the animal turns toward the intact eye continuously, *i. e.*, performs circus movements. If the animals are exposed to light from two sources they go toward a point between them.

[1] Loeb, J., 'Die Bedeutung der Tropismen für die Psychologie,' Leipzig, 1909, 51 pp.

Here we have two criteria of tropisms which are precisely like those given by Bohn. The second one is of particular importance according to Loeb. He says (p. 13) that the difference between the machine-like heliotropic reactions of animals and the non-heliotropic reactions of man lies in the fact that when exposed to light from two sources the former go toward a point between them while the latter goes directly toward one of the two sources of light. "Sind zwei gleich starke Lichtquellen in gleichem Abstand vom Tier vorhanden, so bewegt sich dasselbe senkrecht zur Verbindungslinie der beiden Lichtquellen, weil dann beide Augen in gleicher Weise vom Licht beeinflusst werden. Darin unterscheidet sich, wie *Bohn* richtig bemerkt, die maschinenmässige heliotropische Reaktion der Tiere von der nicht durch Heliotropismus bedingten Bewegung eines Menschen zu einer von zwei Lichtquellen." I have quoted these criteria in full because I shall attempt to apply them later in order to show how extensive reactions are which agree with this definition of tropisms. Loeb claims that they are found in many animals both among invertebrates and vertebrates.

III. Certain animals which do not react to light or which are negative in their reactions, *i. e.*, go from the light, can be made positive by adding a little carbon dioxid to the water or by decreasing the temperature. This the author maintains gives us a 'further insight into the mechanism of the action of the will.' Loeb assumes that the acid acts directly on some postulated chemical substance in the organism, which determines the sense of the reaction. This assumption does very well when applied to copepods, but we meet serious difficulties when we attempt to apply it to Ranatra, which, according to Holmes, becomes negative under any conditions which tend to make it quiet, and positive under conditions which tend to excite it; or to Arenicola larvæ, which I found to become negative in solutions containing various narcotics, acids, alkalies or neutral salts and in concentrations of sea water both below and above normal. The change in sense of reaction in these forms seems to be due to the effect of the environment on the general state of the organism as a whole, rather than to the specific effect of acid on the chemical state of some postulated substance. Moreover, I am unable to see that the regulation of the sense of the reactions in animals by the environment has any bearing on the problem of the will, either from the point of view of the parallelist or from that of the interactionist.

IV. (*a*) The fact that some organisms do not react to light, Loeb says, does not offer any difficulty to the theory of tropisms, nor does

the fact that some orient more accurately in light of a given intensity than in weaker light. All this is in harmony with physico-chemical laws. Thus the manifestations of tropisms depend upon external conditions. But they also depend upon internal changes, *i. e.*, they are variable. This the author thinks is due to the production of acids in metabolism; and just as the addition of acid to the solution causes changes in the reactions to light, so we should expect the acids formed within to produce changes.

(*b*) The reactions to light, the author maintains, are instinctive. They are performed without previous experience. This he thinks proves that orientation is not due to trial reactions. He seems to assume that in accord with the so-called trial and error theory organisms are not able to orient when very young, that they must learn to do this by numerous trials. This statement is evidently directed against the theory of 'trial and error' as applied to the reactions of the Protozoa by Jennings. As I understand this application of the theory, it has, however, nothing to do with the ontogeny of reactions. It does not mean that each individual during its development learns how to orient. It teaches that since orientation in the Protozoa is attained by changing the axial position until the stimulus which causes such changes ceases, *i. e.*, by trial, direct orientation, such as is found in most of the higher animals, has evolved from indirect orientation, the kind found in Protozoa, *i. e.*, orientation by trial. The fact then that certain animals in their embryonic state orient directly has no bearing on this theory as applied by Jennings.

V. Reactions to light, Loeb claims, depend upon the stage of development. Ants are ordinarily negative, but when they are sexually mature they become strongly positive. Similar changes in reactions to light are found in bees and other insects. He admits that these reactions are adaptive, but seems to assume that the nuptial flight of ants and bees and copulation in the air are due to the fact that the animals are positive to light at this time rather than the opposite. He says (pp. 28, 29): "Ich gewann den Eindruck, dass dieser Hochzeitsflug nur die Folge einer hochgradig gesteigerten heliotropischen Empfindlichkeit sei. . . . Die Bienen sind dieser Beobachtung zufolge zur Zeit der Geschlechtsreife positiv heliotropische Maschinen und das Ausschwärmen ist nur die Folge des Heliotropismus." For him it appears to be definitely settled that organisms which live in the light do so because they are positive, and those which live in darkness do so because they are negative. The observations on the insects, he says, show how in the analysis of the phenomena of life tropisms can be

recognized as elements. Now if it really were known that the reactions of an organism determined its general characteristics, its life phenomena, we should have to agree with Loeb in his conclusions. This is, however, in the minds of most men, by no means known. In leaving their nests or hives ants and bees always go toward the light. Light may be the guide which directs their course to the exterior, and if in the order of nature there is any necessity for copulating outside, might not one expect them to go toward the light at the time of sexual maturity? Such reactions might, of course, occur without consciousness.

VI. The author says that reactions to light are not fundamentally adaptive. This conclusion he bases on the supposition that the mechanisms of reactions to light and to the electric current are the same, and on the fact that some organisms which live in the dark are positive to light when exposed. While the reactions to the electric current are undoubtedly not adaptive, very few investigators will agree that the mechanism of reactions to light is like that of the reactions to an electric current, for in some of the Protozoa it is unquestionably different, and in no organism has it been clearly demonstrated to be the same.

VII. Reactions to light, Loeb states, are not generally such as to keep the organism in illumination favorable for its life processes. He maintains that if organisms are positive they are positive in all intensities in which they respond at all. "Es hat sich stets herausgestellt, dass positiv heliotropische Tiere gegen Licht jeder Intensität, sobald nur die Reizschwelle überstiegen wird, positiv heliotropisch sind. . . . Eine 'Auswahl' einer passenden Beleuchtungsintensität habe ich nie beobachtet" (pp. 34, 35). The author admits that positive animals may become negative in strong light, but claims that this has nothing to do with adaptation. The strong light, he assumes, causes chemical changes which make the animals negative. Granting that this is true, I am unable to see how it would show that the reactions are not adaptive. One thing is certain: that is, that *Euglena*, *Volvox*, *Chlamydomonas*, and many similar organisms are positive in weak and negative in strong light, and that this reaction keeps them in moderate light intensity. I have repeatedly seen these organisms collect in dense aggregations in bright spots between aquatic plants on cloudy days, and in shaded places when the sun is bright. They require a certain amount of light, because they depend largely upon the process of photosynthesis for their food. Very intense light however is fatal to them. It seems perfectly obvious that their reactions to light keep them in illumination well adapted to their needs.

VIII. Loeb affirms that there are two essentially different classes of reactions to light. One (tropism) is regulated by the relative rate of chemical reactions in symmetrically located superficial elements (Oberflächenelementen); the other is caused by a rapid change in the rate of chemical reaction in the same tissue. As examples of the latter he cities the retraction of tubicolous annelids when the light intensity is decreased, and says that in *Planaria* a sudden decrease of intensity causes a decrease in locomotion, while a sudden increase causes an increase in locomotion. Ideas, the author maintains, may act like acids in certain organisms in that they may modify the sensitiveness to certain stimuli. He fails however to tell us what ideas are.

I agree with Loeb that certain reactions depend upon the time rate of change of intensity in the stimulating agent, while others depend upon the absolute amount of energy received, but I do not agree with him in this classification. Orientation is, according to Loeb, brought about by tropisms; but orientation in many animals is unquestionably dependent upon the time rate of change of intensity, and in no instance has it been positively demonstrated to be dependent upon the absolute amount of energy received. It therefore unquestionably belongs to the second class in many cases. The rate of locomotion in planarians, however, is, in my opinion, dependent upon the amount of energy received, and belongs in the first class instead of in the second, where Loeb puts it. At any rate, Walter says that sudden change of light intensity does not cause immediate change in the rate of locomotion, but that it does cause the animals to throw the anterior end from side to side, no matter whether the change is an increase or a decrease of intensity.

IX. In this section the author discusses reactions to gravity. However, he has added nothing of importance to a comprehension of his conception of tropisms.

X. The tropisms are elements which make a rational comprehension of psychological reactions possible.

XI. Tropisms, Loeb maintains, are of vital importance not only in psychology but also in ethics. He says that the highest development of ethics is manifested in the sacrifice of life for an idea. This, he claims, cannot be explained on the basis of utilitarianism or on the basis of the categorical imperative. He holds that certain ideas cause chemical changes within the body which increase the sensitiveness to certain stimuli to such an extent that the being becomes a slave to these stimuli and is thus driven to the sacrificial act. Here the author evidently considers the reactions, which are assumed to be determined by

a postulated chemical change caused by a hypothetical idea, to be tropisms. On this basis practically all of the rational acts of a human being might be tropisms. But surely such reactions are not in accord with Loeb's definition of tropisms as presented under II. above.

General Discussion. — If we are to make any use of tropisms it is of course necessary to know how to recognize them. In the definition referred to above Loeb has given us several criteria by means of which this may be done, and Bohn gives the same. Suppose, then, we accept this definition and proceed to apply the criteria so as to learn which reactions are tropisms.

According to the definition, in order that a reaction may be a tropism (1) the stimulus which causes it must be due to constant intensity of the stimulating agent, not to change of intensity; (2) the organisms must turn directly toward one of two symmetrical sides when the intensity of the stimulating agent on them is unequal; (3) if an organism which is exposed to light from two sources goes toward one of them and not toward a point between them the reaction is not a tropism.

If we examine the reactions of the Protozoa, we find that while they go toward a point between two sources of light they always turn toward a given side regardless of the distribution of the intensity of the stimulating agent on the surface. This is true even in forms which are apparently symmetrical, as for example, the ciliate *Didinium*, the flagellate *Chlamydomonas*, and the swarm-spores of *Eudendrium*. According to the definition of Loeb and Bohn there are then no tropisms in the Protozoa.

The insects and vertebrates, probably all animals with image-forming eyes, may turn directly toward one side when opposite sides are unequally illuminated, but when exposed to light from two sources, they usually go directly toward one of them. Moreover when the eye on one side is destroyed so that opposite sides cannot possibly be equally illuminated many of them still orient and travel toward a given source of illumination. This was demonstrated by Rádl for several different flies, by Carpenter for the pomace fly *Drosophila*, by Holmes for the water scorpion *Ranatra*, and by the present writer for the toads. There are then very few if any reactions in animals with image-forming eyes which are fundamentally in accord with the Loeb and Bohn definition of tropism.

In case of the Metazoa without image-forming eyes, cœlenterates, vermes, and larvæ of insects, the reactions may be in accord with the second and third criteria, but in no instance has it been demonstrated that the orienting stimulus is the result of constant intensity as demanded by the theory of tropisms in question.

Can it be then that the tropisms of Loeb and Bohn are of great consequence in the analysis of the behavior of animals, if they are as rare as we have shown them to be? Every investigator will support Loeb in his endeavor to explain phenomena of activity in terms of physical and chemical laws. An objective explanation is the aim of practically every one, but objective explanations must be based upon the results of detailed experiments and observations, not upon expressions which have meanings as multifarious as the term tropism has at present. Recently I have been interested in the various definitions given to this term in scientific literature, and have found fifteen, all slightly different and many radically different. These together with many other references to tropisms will be found in a book on the subject of reactions to light to appear shortly.

Jennings[1] takes a decidedly different view of tropisms from Bohn and Loeb. He defines the term in the original sense in which it was used, *i. e.*, merely to indicate orientation (turning toward) without signifying anything regarding the process by means of which orientation occurs. He says (p. 1): "*The tropism includes those reactions in which the organism takes and maintains a definite orientation — places the axis of its body in a definite position — with relation to some external source of stimulation.*"

Loeb and Bohn assume tropisms to be elements in behavior, the simplest phenomena in the sphere of reactions. Jennings considers them collective concepts. "The tropism," he says, "is not a uniform, elemental thing, identical everywhere, but is a descriptive and collective term for reactions having certain common characters, but differing in other respects" (p. 11). He maintains that the reactions which result in orientation (tropism) are not the same in all organisms. Some turn directly toward the source of the stimulating agent and become oriented; others take many different axial positions before they become oriented. And the same organism may orient differently when stimulated by different agents. The Infusoria, *e. g.*, orient directly in an electric current but indirectly in light. "*The effect of the external agent is visibly and undeniably different in different tropisms*" (p. 13) The electric current causes reversal of the cilia on certain parts of the body; light does not.

Jennings claims further that orientation (tropism) is not necessarily the result of reactions caused by stimulating agents of constant intensity, as Loeb and Bohn maintain. Orientation in the ciliate

[1] Jennings, H. S., 'Tropisms,' *Rapport au VIme Congrès International de Psychologie*, Genève, 1909, 20 pp.

Stentor and in the flagellate *Euglena* in light is unquestionably the result of responses caused by sudden changes in the intensity of the light on the sensitive tissue in the organism. Regarding this question in other forms he says (p. 15): "Whether a sufficiently thorough analysis may not show the same condition of affairs to hold for certain higher animals I will not attempt to say."

I am unable in the limited space allotted this review to do justice to the forceful arguments presented in this address, which, as well as those of Loeb and Bohn, should be read in full by all interested in this subject.

With the discussion bearing on the question of orientation and the criticism of certain tropism concepts, presented in Jennings' address, I am in hearty accord. But I question the advisability of attempting to use the term tropism as defined. If this term could be made to signify to everyone orientation, or any other objective phenomenon without reference to the cause of such phenomena, there might be some advantage in retaining it; but it is used at present in so many senses and in many cases with such deep-rooted hypothetical causal significations that I fear it would be impossible to bring this about, no matter how one might choose to define the term. It seems to me that thorough analysis, clear thinking, and mutual understanding require the elimination of the use of the term tropism, at least for the present. If, for instance, tropism means orientation, why not use 'orientation,' a term which has a definite meaning in behavior, and is not burdened with metaphysical causal appendages?

2. Miscellaneous Papers.

1. 'Selection of Food in *Stentor cæruleus* (Ehr.),' by A. A. Schaeffer, *Jour. Exp. Zoöl.*, Vol. 8, pp. 839–896. A year ago I reviewed an abstract of this paper in this journal. The essential points in the paper were presented then. I refer to it now only to record the place of publication.

2. 'The Reactions of Sponges, with a Consideration of the Origin of the Nervous System,' by G. H. Parker, *Jour. Exp. Zoöl.*, Vol. 8, pp. 1–41. This interesting paper deals primarily with the origin of the nervous system. The author attacked the problem from a physiological point of view. His aim was to ascertain whether there is any physiological evidence indicating that sponges have a nervous system. In order to do this the reactions of sponges to various stimuli were studied. The sponge is a comparatively inactive animal. There is no motion in it as a whole, but there are flagella in the cavities

which beat and produce currents; muscle cells, myocytes, which close the openings of the cavities, the ostia and oscula, and muscle fibers scattered through the flesh, which may contract and give the sponge a shriveled appearance. By means of proper stimuli the ostia and oscula could be made to close and open, the flesh could be made to contract, and the beating of the flagella could be made to decrease or increase but not to reverse. There was no response to light or mechanical stimuli, except to injury. The response in all instances seems to be due almost entirely to the direct effect of the stimulating agent on the reacting tissue. There is very little evidence of transmission of impulses. "Such transmission as is present is so sluggish in character and so slight in range as to resemble transmission in muscles and not in nerves. It is probable that [the sponge] *Stylotella* possesses no organs that can reasonably be called nervous.

"The nervous and muscular systems of metazoans were not differentiated simultaneously . . . nor independently, . . . but muscles, independent effectors, as represented by the sphincters of sponges, were the first of the neuromuscular organs to appear and these formed centers around which the first truly nervous organs, receptors, in the form of sense-cells developed, giving rise to a condition such as is seen in the cœlenterates today. To this receptor-effector system as seen in modern cœlenterates was added in the higher metazoans the adjuster or central organ, thus completing the essential parts of the neuromuscular mechanism as seen in the higher metazoans" (pp. 39, 40).

3. 'Some Light Reactions of the Medusa *Gonionemus*,' by L. Murbach, *Biol. Bull.*, Vol. 17, pp. 354–368. The light reactions of *Gonionemus* have been quite thoroughly investigated by Yerkes and Morse. There are a few points on which these authors do not agree, and it is these points with which the experiments and discussion of the paper under consideration primarily deal. Taking into account the experimental results of all the work on *Gonionemus* it may be safely concluded that the following points are established.

(*a*) *Gonionemus* under certain conditions orients in light, but rather indefinitely. Orientation however plays only a very insignificant part in its behavior.

(*b*) Sudden changes of intensity in either direction tend to make these animals active. In light of relatively low intensity they tend to come to rest, while in that of relatively high intensity they tend to remain active. This causes them to collect in light of moderate intensity.

(*c*) The rate of locomotion apparently depends upon the absolute amount of light energy received. A sudden change of intensity causes a definite response consisting of an abrupt change in the rate or direction of locomotion.

A number of other reactions in the habits of this extremely interesting organism are recorded in the paper. These however we shall not attempt to present here.

4. 'The Reactions of *Æolosoma* (Ehrenberg) to Chemical Stimuli,' by H. G. Kribs, *Jour. Exp. Zoöl.*, Vol. 8, pp. 43–74. This paper contains an account of the reactions of *Æolosoma* to mineral acids, organic acids, hydrates, carbonates, halides, and sulphates, of various concentration, when applied to different parts of the body. It was found that if very weak chemical solutions are applied to one side of the anterior end, the animal responds by turning toward the side stimulated. After giving this reaction it may however swing the head from side to side. If a stronger solution is similarly applied the animal always turns directly from the side stimulated. (We apparently have here a differential response to a localized stimulation.) Both of these reactions are adaptive. The first under normal conditions brings the animal in contact with substances which may serve for food. The second carries it away from injurious substances. These reactions are similar to the positive and negative reactions in *Planaria* caused by weak and strong mechanical stimuli (Pearl). If the chemical is applied to the central part of the body, both the posterior and the anterior ends are turned ventrally and approach each other regardless of the location of the point stimulated. If however the stimulation is weak the anterior end may turn toward either side.

There are unfortunately many typographical errors in this paper, and some peculiar expressions, *e. g.*, "solutions which stimulate a reaction," and "the stimulus impinges laterally to the prostomium."

5. 'Further Observations on the Behavior of Tubicolous Annelids,' by C. W. Hargitt, *Jour. Exp. Zoöl.*, Vol. 7, pp. 157–187. The tubicolous annelids are worms which dwell in tubes open at one end. From this end the gills of the animals usually project. When the light intensity is suddenly decreased they usually retract and remain in the tubes for some time before reappearing. The variability in this time and in other features in the reactions under the same external conditions is the principal theme of the paper.

The observations were made on some individuals collected in water from 8 to 15 fathoms deep, and on others collected in shallow

water. The stimulus in every instance consisted in turning off an electric bulb at a constant distance. In the former the response was very irregular, *e.g.*, one individual responded the third time the light was turned off and remained in the tube 18 seconds; then it did not respond again until the eleventh time, when it remained in the tube 35 seconds, after which it did not again respond during the remaining 13 trials. Those collected in shallow water responded much more definitely, but the time they remained in the tubes varied greatly in different individuals, and in the same individual after successive stimulations; *e.g.*, one remained 6 seconds after the first time the light was turned off, 5 seconds after the second time, 10 seconds after the third, 9 after the fourth, etc. Several times it did not respond at all. Another individual remained 360 seconds after the first stimulus, 60 after the second, 180 after the third, 50 after the fourth, etc. These results agree with those of Mrs. Yerkes (1906).

The difference in the response of animals taken from different depths, together with observations on colonies which had been subjected to frequent shadows in the laboratory for several months, lead the author to believe that the behavior of these animals is modifiable. The variability in the response of a given individual mentioned above, he assumes, cannot be accounted for as being due to the stimulus or any other environmental factors. This leads him to conclude that these organisms are not 'mere machines, automata.' "It is more than ever evident that, though we admit the intimate relation of chemical and physical processes to every aspect of behavior, they are not of themselves explanatory except in the most partial way. Indeed it may well be questioned whether the all but universal disposition to explain the organic in terms of mechanics is not an actual reversal of the natural sequence of relations. This has grown out of the assumption of the derivation of life from the non-living. Who shall say that the reverse may not have been the order of evolution, and that the inorganic may not be better interpreted in terms of organic life?" (pp. 184, 185).

There can be no question as to the reliability of the results of Hargitt's experiments, nor can there be any question as to the validity of the assumption that the reactions of an organism cannot be accounted for on the basis of present environmental conditions; nor can it be questioned that for all that is known to the contrary, psychic factors may be involved in behavior; and since we do not know what life is, how can we question that it may have preceded matter in the process of evolution? But I am unable to see how this shows that the reac-

tions cannot be mechanical, *i. e.*, subject to chemical and physical laws. I am well aware that the activities of organisms have not been explained in terms of mechanics, and I agree with Hargitt that they cannot be accounted for on the basis of present environmental conditions, but this does not mean that we should not have a complete mechanical explanation of every living organism if we knew all about the physical and chemical states and changes within the organism and the effect of environment on them. But it should be emphasized that even if we did know all this, and were able to account for every reaction mechanically, even to the extent of chemically synthesizing organisms with specified reactions, we should know nothing regarding the origin of life or its cause; nor should we know the cause of the reactions, excepting in so far as the observed sequence of events associated with the reactions may be assumed to be their cause.

Variability in response to the same stimulus, such as Hargitt observed, seems to me to show that reactions are not dependent upon external conditions alone, and that if they are the result of these conditions together with the physiological states and changes without any non-mechanical factors, the physiological changes are not continuing with any degree of regularity.

6. 'Factors Determining the Reactions of the Larva of Tenebrio Molitor,' by Max Morse, *Jour. Comp. Neurol. & Psychol.*, Vol. 19, pp. 721–729. *Tenebrio* larvæ orient fairly accurately and go from the light. The author maintains that orientation is entirely controlled through the eyes or eye-spots. He found that it takes place in larvæ with one eye blinded nearly as accurately as in normal specimens. This is perhaps the most important result obtained. It shows that the theory of heliotropism as defined by Loeb and supported by Bohn does not hold for this animal, and the same is true for a number of others as pointed out elsewhere in this review. "It is evident," the author says, "that this larva, low in the scale as it is, presents highly complex behavior, the factors of which are but slightly known."

7. 'Ueber Orientierung, Lokomotion und Lichtreaktion einiger Cladoceren und deren Bedeutung für die Theorie der Tropismen,' by W. F. Ewald, Erlangen, 34 pp. Many of the Cladocera orient with the eye always in a given relation to the source of light, no matter whether they are negative or positive. If the direction of the rays of light is changed when the body is held so that it cannot move, the eye turns. Orientation is thus evidently regulated through the eye. This was observed by Rádl and confirmed by Ewald. The paper under consideration deals however primarily with depth migration. The author

found that if light from above is suddenly decreased, the animals come nearer to the surface, and if it is decreased they go down, but that they soon become adapted to the change and resume their original position. The stimulus, the author maintains, is the change of intensity, not the difference of intensity owing to the change.

8. 'Versuch einer Analyse des Instinkts nach objectiver vergleichender und experimenteller Methode,' by Romuald Minkiewicz, *Zool. Jahrb.*, Vol. 28, pp. 155–238. A number of different crabs are known to fasten seaweeds and other objects to their carapace. The process involved in this is apparently instinctive, *i. e.*, there is no improvement with practice. Minkiewicz claims that under certain conditions some crabs take for the purpose only those objects which match the environment in color, and maintains that he has analyzed the process involved in this apparent selection and disguisement which is supposed to be an instinct. The facts underlying this analysis have been presented in this BULLETIN in the reviews of his original papers, some by Jennings and others by myself. There is consequently no need of repeating them here. In certain quarters there has been doubt as to the accuracy of the results presented in these papers. They are of great importance and should be confirmed if possible at the earliest opportunity. The author has introduced in his analysis, numerous new terms (Erythrotropismus, Apochromatismus, Synchromatismus, chromokinetischer Resonanz, etc., etc.), which, in my opinion, are a hindrance in presenting an objective analysis rather than a help. Terms of this sort are invariably interpreted by many as indicating metaphysical causation, whereas from an objective point of view they are intended to indicate only observed phenomena.

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RECENT LITERATURE ON THE BEHAVIOR OF THE HIGHER INVERTEBRATES.

VON UEXKUELL CRITICISED.

Although most of von Uexkuell's researches have been upon the lower invertebrates, yet his conclusions are so far-reaching as to make Jennings' review [1] of his work of intense interest to all students of animal behavior. Jennings endorses the truth of v. Uexkuell's principle that the first requirement for successful biological investigations is "the continued and accurate observation of the living animal in its

[1] Jennings, H. S., 'The Work of v. Uexkuell on the Physiology of Movements and Behavior,' *Jour. of Comp. Neurol. and Psychol.*, Vol. 19, 1909, pp. 331–336.

environment." He admits that v. Uexkuell has probably done more towards an analysis of the internal processes of the lower animals than any other man. Nor does he condemn the invention of a fictitious schema of main nerve tubes, feeders, reservoirs, valves, etc., for conducting, storing, and controlling a peculiar fluid known as 'tonus,' which causes the tension and contraction of muscles. If such a schema will clarify complex behavior there can be no objection to it: but, believing that "The object of science is to prepare a system of verifiable propositions, in order to know what we may depend on in our conduct," Jennings contends that in his written works v. Uexkuell should sharply separate what is merely fictitious from what is or may be verifiable. According to Jennings, v. Uexkuell lays too much stress upon making things 'anschaulich' (graphic), and this has led him into vitalism; for the development of an individual from the germ cannot be rendered 'anschaulich.' Over-emphasis of this method has prompted v. Uexkuell to desire the divorce of physiology from biology, and has made him a kind of double personality — a vitalist in embryology, a mechanalist in physiology. Jennings rightly holds that verifiableness, not 'Anschaulichkeit,' should be our aim. He writes: "Taking verifiableness as our aim will likewise leave biology and physiology resting peacefully in union. We should be interested in the plan of the organism so far as it is verifiable; and to work out the verifiable plan we should be forced to consider the actual forces, materials, and arrangements, not fictitious ones. Doubtless physiology has in practice become narrowed: the remedy lies in broadening it till it includes everything verifiable in the study of the processes of organisms."

Tropisms.

Comes[1] has conducted a series of experiments which he thinks prove that the larval ant-lion is positively stereotropic, positively geotropic, and within certain limits, positively thermotropic. These tropisms and simple reflexes, according to Comes, are sufficient to explain all of the phenomena connected with the burrowing and feeding of this insect, activities which previous observers have considered instinctive.

Kellogg, who, several years ago,[2] demonstrated that the male silkworm finds the female exclusively by a chemotactic response to the

[1] Comes, Dr. Salvatore, 'Stereotropismo, geotropismo, e termotropismo nella larva di *Myrmeleon formicarius* L.,' *Atti d. Acad. Gioenia d. Sci. nat. in Catania*, Anno LXXXVI., 1909, Serie quinta, vol. 2, Memoria IV., 14 pp.

[2] Kellogg, V. L., 'Some Silkworm Moth Reflexes', *Biol. Bull.*, vol. 15, 1907, pp. 152–154.

odor of her scent gland, and recently[1] has recorded experiments showing that the swarming of the honey bee is due to a sudden intensifying of positive heliotropism, although feeling confident that many animal movements are tropisms, is not a subscriber to the extreme mechanicalism of Bethe and his followers; for he says: "Our point of view will be, however, as stated, that fairly safe one between the rigid mechanicalism of the tropism believers and the mysticism of the believers in a divinely endowed creature of psyche as contrasted with a long series of unfortunate soulless brutes."

Sensations.

1. *Vision.* — Although some have claimed that the facetted eyes of insects enable them to have images, and others that these eyes enable insects merely to distinguish objects in motion, yet the majority of both sets of thinkers agree with Mueller, Lubbock, Exner, and others, that each element conveys to the sensitive region in the rear of the eye a ray of light which comes from one point of the outer world, and that in the eye, these points form a mosaic. Long ago, Gottsche (1852), Dor (1861), and others opposed this view; but their objections have been considered invalid. Recently Vignier[2] has revived, in a modified form, Gottsche's contention that the elements of the facetted eye form images. The retinula of each ommatidium of a fly's eye is composed of seven elements, one of which forms an axis around which the other six are grouped. At their proximal ends the peripheral elements converge towards the central one. The proximal end of each of these elements is connected with a separate nerve fiber. According to Vignier, each of these elements forms, at its proximal end, a small, inverted image of a portion of the field of vision; thus each ommatidium, instead of transmitting to the sensitive portion of the eye one point of light, converges upon it seven inverted images. Since these images are inverted, it would be impossible for a composite image of an object to be formed in the eye. The seven nerve fibers of each ommatidium leave the eye as a single fasciculus. In the periopticum each fasciculus is rotated on its axis through an angle of 180° and its fibers spread out a little; and those fibers of adjacent ommatidia that convey impressions of the same portions of the object are fused. In this way an erect neurophotogramme is formed. This gives a mosaic formed, not in the eye, but in the periopticon.

[1] Kellogg, V. L., 'American Insects,' Henry Holt & Co., 2d edition, revised, 1908, 694 pp.

[2] Vignier, M. P., 'Mécanisme de la synthèse des impressions lumineuses recueillies par les yeux composés des diptères,' *C. r. Acad. Sci., Paris*, t. 148 (1909), pp. 1221–1223.

For many years biologists have held that all pentamera beetles have eucone eyes. [Eucone eyes are facetted eyes each ommatidium of which is provided with a crystalline cone.] Recent researches of Kirchhoffer[1] have demonstrated that the following genera of pentamera beetles do not have eucone eyes: Staphylinidæ, Histeridæ, Silphidæ, Malacodermidæ, Cleridæ, Byrrhidæ, Elateridæ, and Dermestidæ. The first three families have acone eyes; what has been mistaken for a crystalline cone in the others is a portion of the cornea. These data will be of value to students of the visual sensations of beetles.

2. *Sensations of Ants.* — After reviewing the work that has been done on the sensations of ants, Wheeler[2] arrives at the following conclusions: (1) Bethe's terms, photoreception instead of seeing and chemoreception instead of smelling, etc., are undesirable, because there is no sound reason for their use. (2) The sense of touch is located in tactile hairs on the antennæ and, probably, in the hairy portion of the chitinous integument. This sense has not been thoroughly studied in ants. (3) That ants have a delicate temperature sense is evidenced by the way they regulate their hours of activity and by the way they place the brood in the best situation for utilizing the warmth of earth and stones. The organ of this sense is unknown. (4) That ants are capable of feeling pain is evidenced by the fact that they exhibit unequivocal signs of discomfort, especially when the antennæ are pinched or when they are touched with certain corrosive or strongly irritating substances; but it must differ profoundly, both in quality and intensity, from that which we suffer when undergoing operations. (5) The principal organs of smell are probably the prostrate, club-shaped sensillæ of the antennæ. Ants perceive odors diffused in air as well as those dissolved in liquids. When the antennæ come in contact with a body, the tactile and olfactory impressions fuse, forming what Forel calls topochemical sensations. Miss Fielde's claim that the different antennal joints are specialized for detecting different odors is based on insufficient and unsatisfactory evidence. The structure of the antennæ does not lend the slightest support to it. (6) Forel demonstrated that when morphine or strychnine was mixed with honey the ants did not detect it until the mouth parts had touched it, thus

[1] Kirchhoffer, Otto, 'Untersuchungen über die Augen pentamerer Kaefer,' *Archiv für Biontologie*, Bd. 2, 1909, pp. 237–287, Pl. XVI.–XXII.

[2] Wheeler, W. H., 'Ants, Their Structure, Development and Behavior,' Columbia University Press, 1910, 663 pp., 286 figs. This is by far the best popular book on ants that has appeared in any language. Its intrinsic merit appeals, not only to lovers of nature, but to all serious students of the morphology, taxonomy, and behavior of ants.

proving that ants have a sense of taste. There are rows of sensillæ on the maxillæ and at the base of the tongue which appear to be the organs of this sense. It is in this sense that the ants most nearly approach the higher animals and man. (7) The reactions of the eyes of ants to light are both phototropic and visual, but vision is poorly developed in the female and worker. (8) There is an abundance of evidence to prove that ants are affected by sound vibrations and some indication that these vibrations may be received through the air; but the evidence of the latter is not sufficiently strong to be convincing. [On this point we differ from Wheeler, for, to say nothing about the work of other investigators, we feel sure that one of Wheeler's own experiments demonstrates that ants respond to aerial vibrations produced by sounding bodies.[1]]

Mating and Nest Building Instincts.

1. *Mixed Colonies.* — The myrmecological literature contains a few records of mixed colonies of *Formica* and *Lasius*. Wasmann[2] attempted to form such a mixed colony by giving to a nest of *Formica rufibarbis* numerous cocoons of *Lasius niger*. These cocoons were taken into the nest and adopted. In less than a month one *Lasius* worker emerged and remained in the nest for two days, when it was killed by *F. rufibarbis* workers. The pupæ in the other cocoons did not emerge. This led Wasmann to conclude that the accidental development of *Lasius*, in the nest of *Formica*, out of cocoons which have been brought in as booty, is a fable which is not realized in nature. This is a universal conclusion drawn from a single experiment. Wasmann also asserts that what have been mistaken for mixed colonies of *Formica* and *Lasius* are but juxtaposed nests.

By adding the cocoons of *Lasius niger* and *Lasius alienus* to a weak colony of *Lasius niger*, Wasmann succeeded in forming a mixed colony of these two species of *Lasius;* but, partly because of his knowledge of the habits of ants and partly because of test experiments made in the field, he is convinced that, in most cases, mixed colonies of species of *Lasius* are the result, not of the raising of young from captured cocoons, but of the adoption of a queen of one species by a weak colony of another species.

As the result of observations in the field and of laboratory experi-

[1] Wheeler, W. H., *ibid.*, p. 513.

[2] Wasmann, E., 'Ueber gemischte Kolonien von Lasius-Arten,' *Zool. Anz.*, Bd. 35, 1909, S. 129–141.

ments, Viehmeyer[1] concludes that it is difficult for the female *Formica sanguinea* to raise her brood unaided, and that an alliance between *Formica sanguinea* and *F. fusca* is not improbable.

In chapter XXIII. of his new book,[2] Wheeler gives a critical résumé of much of the work that has been done on mixed colonies of ants.

2. *Mating Behavior.* — Among species of ants the males and females of which are provided with wings, it is generally believed that mating always occurs in the air. Recent observations and experiments[3] have demonstrated that, at the time of the nuptial flight, the female *Pogonomyrmex badius* wanders about on the top of the mound of the nest and mates with males that come from other nests. Not only were males arriving at the nest noticed to mate with females wandering about there; but whenever males captured on their arrival at the mound were confined in test-tubes with females that had just emerged from the nest, mating occurred.

Social Instincts.

Wasmann[4] found *Staphylinius stercorarius* in several nests of *Tetramorium cæspitum*, but never in the nests of other ants nor under stones not covering a nest of the species mentioned. By experiments conducted in artificial nests, he has demonstrated that during the day the beetle lies concealed in burrows that it makes in the walls of the nest, and that at night it roams about the nest and feeds upon the worker ants and pupæ. Wasmann also found that *Staphylinius fossor* bears the same relation to *Formica sanguineus* that *S. stercorarius* bears to *T. cæspitum*.

Wheeler[5] devotes chapters XXI. and XXII. to a discussion of guests found in ant nests. Those interested in the subject will find these chapters worth reading.

Miscellaneous Instincts.

1. *Fighting Instincts.* — It is generally believed that the female bee never uses her sting as such except against a rival female bee.

[1] Viehmeyer, H., 'Beobachtungen und Experimente zur Kolonien-Grundung von *Formica sanguinea* Latrl.,' *Zeit. für wiss. Insektenbiologie*, Bd. 5, 1909, S. 353–356, 390–394.

[2] *Op. cit.*

[3] Turner, C. H., 'The Mound of *Pogonomyrmex badius* Latrl., and its Relation to the Breeding Habits of the Species,' *Biol. Bull.*, vol. 17, 1909, pp. 161–169.

[4] Wasmann, E., 'Staphylinius-Arten als Ameisenrauber,' *Zeit. f. wiss. Insektenbiologie*, Bd. 6, 1910, S. 1–10.

[5] *Op. cit.*

Waetzel[1] has noticed a recently emerged queen sting to death a male of the same hive. So far as I know this is the only record of such an occurrence.

2. *Hibernation.* — Newell and Dougherty,[2] as the result of a series of well conducted experiments, believe Hunter and Hinds in error when they state that at a mean temperature of 60°[3] the majority of the boll weevils hibernate; for they found the weevils active when the temperature had fallen to 41.7°. Newell and Dougherty think that perhaps a certain minimum temperature induces hibernation. In Louisiana, by the end of December, the majority of the weevils were hibernating.

3. *Instincts of Ants.* — Wheeler[4] occupies chapter XXIX. with a critical discussion of the instincts of ants. The data have appeared elsewhere, and most of them have been reviewed in past issues of this journal. Wheeler believes that, except in one-celled organisms and in the separate cells of metazoans, instincts are something more than compound reflex actions, and agrees with Forel and Wasmann that the instincts of ants show unequivocal signs that these insects possess both feelings and impulses. To use his words: "In my opinion they [ants] experience both anger and fear, both affection and aversion, elation and depression in a simple, 'blind' form, that is, without anything like the complex psychical accompaniment which these emotions arouse in us."

Homing.

During the past few years there have been reviewed in this magazine several papers treating of the homing of bees and related insects. Some of these claim that homing is a result of the bees' keen vision of distant objects, others that the bees are directed by odors, and yet others that they are guided by memories of the environment. Bethe has stood practically alone in insisting that, up to a distance of three kilometers, bees are led home by an unknown power. Bonnier[5] has published an article which Bethe will consider a confirmation of his contention. Bonnier found that bees, the eyes of which had been rendered opaque by means of pigmented collodion, would pass direct to

[1] Waetzel, Paul von, 'Biologische Beobachtungen am Bienenstaat: Erstechen einer Drohne durch eine frischausgeschluepfte Koenigin.'

[2] Newell and Dougherty, 'The Hibernation of the Boll Weevil,' *State Cotton Pest Commission of Louisiana*, Cir. 31, 1909, pp. 163–219.

[3] Since the F. scale is used in several of their tables, I presume these are F. degrees, but the text does not state what scale is used.

[4] *Op. cit.*

[5] Bonnier, Gaston, 'Le sens de la direction chez les abeilles,' *C. r. Acad. Sci., Paris*, t. 148, 1909, pp. 1019–1022.

the hive from any distance less than three kilometers. By means of the following experiment, Bonnier seems to have demonstrated that bees can distinguish between places that differ from each other by a small angle. One evening he placed a table 200 meters from a hive. On that table he placed dead branches that had been smeared with cane syrup. In the morning certain bees began to make regular visits to the twigs. These bees were marked with a green powder. That evening a similar table was set at a distance of six meters from the hive. The following morning certain bees began to visit this table. These bees were marked with a red powder. The twigs on both tables were kept smeared with syrup, and bees visited each regularly; but, with rare exceptions, bees that visited one table did not visit the other. These experiments led Bonnier to conclude that bees are guided home, neither by odors nor by vision, but by a 'sense of direction' similar to that possessed by pigeons. This sense is not located in the antennæ. Its seat is probably in the cerebral ganglion. The experiments certainly show that blind bees can find the way home; but they do not demonstrate that visual impressions do not assist normal bees to find the way home.

In the homing of ants, Wheeler[1] holds that the topochemical or contact-odor sense is all-important, but that there is no such mechanical following of the odor trail as Bethe claims; other senses are relied upon to some extent, and memory plays a part.

Plastic Behavior of Ants.

Wheeler[2] devotes chapter XXX. to a discussion of the plastic behavior of ants. Even those who may not agree with all of his conclusions must admit that this chapter is a valuable critical résumé of that subject. His conclusions may be epitomized as follows: (1) In ants there are two kinds of plastic behavior, random (trial and error) movements and what some authors have called 'associative memory.' (2) "The ant's ability to distinguish between 'friend' and 'enemy' does not depend upon inherited reflexes, but on a sensory perception of the olfactory impressions she receives during the first days of her life as an imaginal worker." (3) Ants communicate by both antennal signs and stridulation. [We agree with Wheeler that one cannot watch ants continuously without feeling that they can communicate. At the same time, we must admit that all attempts to prove experimentally that ants can communicate by means of antennal signs have

[1] *Op. cit.*
[2] *Op. cit.*

yielded negative results.] (4) Imitation and coöperation as a result of imitation are common among ants. (5) Ants have memory and learn by experience. They have images and ideas; but, in all probability, these do not bear the slightest resemblance to our own, the main content of each idea being topochemical. (6) It is improbable that ants do any reasoning. Most cases of supposed acts of reasoning may be explained in simpler terms; about those recorded cases which we cannot now explain in simpler terms we had better suspend judgment 'till careful experimental data are available.'

In discussing the origin of instinct, Wheeler remarks: "It is . . . quite futile to attempt a phylogenetic derivation of automatic from plastic activities or *vice versa*, for both represent primitive and fundamental tendencies of living protoplasm, and hence of all organisms. As instinct, one of these tendencies reaches its most complex manifestation in the Formicidæ, while the other blossoms in the intelligent activities of men."

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RECENT LITERATURE ON THE BEHAVIOR OF VERTEBRATES.

In the following summary of the year's literature on vertebrate behavior it has not been possible to give more than the most important result of each piece of work.

Fish. — Sheldon[1] has shown that the skin sensitiveness of the dogfish to acid, alkali, salt, and bitter stimuli is related to 'common sensation,' by proving that it is dependent not on the olfactory nerves but on the maxillary ramus of the trigeminal, a nerve of common sensation. He argues that smell and taste have developed from a form of general sensibility, and thinks it probable that bitter was the earliest differentiated taste.

Bauer,[2] in a paper on successive brightness contrast in fish, continues some earlier attempts to interpret the light responses of lower animals in terms of the Hering theory. The reviewer confesses her failure to understand his argument in the present paper. The fish experimented on were young of the species *Smaris alcedo*. When light is admitted at one side of the vessel containing them the fish

[1] R. E. Sheldon, 'The Reactions of the Dogfish to Chemical Stimuli,' *Jour. Comp. Neurol. and Psychol.*, vol. 19, p. 273, 1909.

[2] V. Bauer, 'Ueber sukzessiven Helligkeitskontrast bei Fischen,' *Zent. f. Physiol.*, Bd. 23, S. 593, 1909.

swim towards it. This response ceases and the fish distribute themselves through the vessel as they become adapted to the light. Now if the light is very bright, and if the fish after having become adapted to it are subjected to moderate light, they will after a brief interval go to the dark end of the trough. This the author thinks is a movement towards the negative after-image, which he says would show blackest against the dark end. But why should positive animals move towards a black image? And if they have become negative, they would go to the dark end without any after-image. If, as is apparently not the case, it were meant that the after-image would look bright against the dark end, thus attracting positive animals, it still would not look as bright as the light end of the box. The presence of an after-image seems undemonstrated.

Amphibians. — Skin sensitiveness to light has been proved in various species of amphibians by Pearse.[1] Their reactions are without random movements, and are determined by the difference in intensity of illumination of the two sides of the animals rather than by direction of rays. When colored light was used, the violet end of the spectrum was most and the red end least effective in determining the responses of animals with eyes; but all colors were equally effective in producing reactions in eyeless animals. This is important, as a corollary would seem to be that skin sensitiveness to light in these animals cannot be sensitiveness to the ultra-violet rays.

Reptiles. — The study of a lizard in its normal habitat which Newman and Patterson[2] have given us would be a valuable supplement to experimental work on the species, but its results are not sufficiently analyzed to be stated in brief.

Birds. — The behavior of six species of altricial birds from hatching to the time of leaving the nest has been studied by Kuhlmann[3] with the aid of a mirror placed above the nest. His most important results concern the relations of the food reaction and developing fear. The former is at first given to any kind of stimulus. Through the influence of experience the bird comes to inhibit it with regard to the various artificial stimuli used. This learning process is complicated by the growth of fear reactions, which are given in time to all the artificial stimuli. The author concludes that the effect of experience

[1] A. S. Pearse, 'The Reactions of Amphibians to Light,' *Proc. Amer. Acad. Arts and Sciences*, vol. 45, p. 161, 1910.

[2] H. H. Newman and J. T. Patterson, 'Field Studies of the Behavior of the Lizard *Scoloporus spinosus Floridanus*,' *Bull. of the Univ. of Texas*, no. 137, 1909.

[3] F. Kuhlmann, 'Some Preliminary Observations on the Development of Instincts and Habits in Young Birds,' *Psych. Monographs*, vol. 11, p. 49, 1909.

on fear is less that animals come to fear particular things with which they have had an unpleasant experience than that they continue to fear everything with which they have not had favorable experience.

Porter's[1] principal contribution in his paper on intelligence and imitation in birds is his method of studying imitation. Instead of requiring an animal to learn, by observing another animal, an action that is wholly new to it, he first allows a bird to learn to open a box in its own way. A second bird is then set the problem, the first one watching. Bird number two probably solves it in a somewhat different way, and the question at issue is whether bird number one will at a later trial modify his method in the direction of that used by bird number two. This mode of procedure, the author thinks, brings in more 'rivalry and affective interest' than is the case where bird number one has had no experience before being required to imitate. Tests on twelve species of birds resulted in some apparent instances of this type of imitation.

Hadley[2] records the case of a cockerel that had learned to work an automatic feeder; the learning process itself, however, was not observed.

Mammals.—Yerkes,[3] continuing his studies of the dancing mouse, finds no very marked differences in the power to learn the black-white discrimination correlated with differences in age and sex. The very interesting result that the optimal strength of electrical stimulus for learning decreases as the difficulty of discrimination increases is confirmed.

Richardson's[4] 'Study of Sensory Control in the Rat' is especially noteworthy in furnishing a new form of test, designed to compel the use of vision in meeting it. The rats were taught to jump from one platform to another, the distance between the two platforms being gradually increased. Normal rats learned to jump 22 inches; two blind rats learned to jump 11 inches, while two other blind rats failed entirely. The test was then complicated by varying the distance and the direction of the jumps; and it was found that while normal rats could accommodate themselves to changes in direction, they failed to

[1] J. P. Porter, 'Intelligence and Imitation in Birds,' *Am. J. Psych.*, vol. 21, p. 1, 1910.

[2] P. B. Hadley, 'Notes on the Behavior of the Domestic Fowl,' *Am. Nat.*, vol. 43, p. 669, 1909.

[3] R. M. Yerkes, 'Modifiability of Behavior in its Relations to the Age and Sex of the Dancing Mouse,' *Jour. Comp. Neurol. and Psychol.*, vol. 19, p. 237, 1909.

[4] Florence Richardson, 'A Study of Sensory Control in the Rat,' *Psych. Monographs*, vol. 12, p. 1, 1910.

adapt themselves to sudden reduction in the distance of the jump. It is concluded that their vision served for the perception of direction, but not for accurate distance perception.

The most novel feature of Yoakum's[1] work on squirrels lies in his experiments on the temperature sense. The apparatus consisted of two hollow-sided boxes whose sides could be filled with water at a desired temperature. Food boxes were sunk in the floor, so as to be invisible, at the further ends of the boxes. It was shown that the squirrels could discriminate a difference of twenty-five degrees, and one animal succeeded in distinguishing a difference of ten degrees.

Cole and Young[2] have shown that the raccoon can discriminate glasses covered with papers of different colors approximately equal in brightness for human vision. Smell was carefully excluded as a source of error. The probability that the discrimination was made in terms of brightness was lessened by showing that the animals did not discriminate grays very well. The fact that the colors distinguished were equal in brightness for the human eye is irrelevant, as their brightness value to the raccoon might have been quite different.

Colvin and Burford[3] recognize that they have not wholly eliminated the brightness error in their experiments on three dogs, a cat and a squirrel. Painted puzzle boxes were used: the colors were two reds differing in saturation, green, blue, yellow, orange, violet, red-orange, and red-red-orange. They were all matched with 'a medium gray, which was also used for one of the boxes. The method employed lay in teaching the animals to discriminate a standard red from each of the other pigments successively, and then when his training was complete to require him to pick out the red from all the other boxes presented simultaneously. In a later series with one dog blue, yellow, and green were used as standards. All the boxes were successfully discriminated. The authors say, "The tests seem clearly to show a surprising fineness of color discrimination among the animals tested." This is in spite of the previous admission that the brightness error had not been eliminated. "The averages show that the red-orange was more successfully discriminated from the standard red than was the blue, yellow, green, and violet." This is indeed surprising, and taken together with the fact that the discrimination of red from red-orange

[1] C. S. Yoakum, 'Some Experiments upon the Behavior of Squirrels,' *Jour. Comp. Neurol. and Psychol.*, vol. 19, p. 541, 1909.

[2] L. W. Cole and F. M. Young, 'Visual Discrimination in Raccoons,' *Jour. Comp. Neurol. and Psychol.*, vol. 19, p. 657, 1909.

[3] S. S. Colvin and C. C. Burford, 'The Color Perception of Three Dogs, a Cat, and a Squirrel,' Studies from the Psych. Laboratory of the Univ. of Illinois, *Psych. Monographs*, vol. 11, p. 1, 1909.

was almost the last one the animals had to learn, it suggests that they may possibly have been helping themselves out by some other clue than color, the movements of the experimenter, perhaps. Another set of experiments was performed in which the cat and two of the dogs were taught to react to red vessels of various forms, the aim being to secure abstraction of the color red from the shape and size of the box.

It is interesting to find that Pawlow's method is among certain investigators to such a degree the orthodox way of studying the animal mind that they apologize for making use of a 'muscle-reflex,' instead of the salivary reflex, as material for observation. Zeliony[1] trained a kitten to come for food when a C' whistle was blown in another room, and found by mingling the stimulus tone with others of different pitch that the cat could discriminate a difference of a half-tone. He suggests that as in the case of the salivary reflex the intensity of the excitation may be measured by the number of drops of saliva, so in such a 'muscle-reflex' as running to get food it may be estimated by the promptness of beginning the reaction, the speed of running, the directness of the course, and the distance the animal runs before checking itself in the case of a wrong stimulus.

Haggerty,[2] working with eleven Cebus and Macacus monkeys and using a number of devices whose manipulation was to be learned, has succeeded in collecting sixteen cases of what appears to be inferential imitation. Instinctive imitation seemed not to be common; the monkeys 'did not display much tendency to repeat the mere acts of other monkeys,' a fact which of course increases the probability that the observed cases of imitation were inferential. The author distinguishes five 'levels of imitative behavior'; mere looking or arrest of attention, following, going to a particular locality, going to a particular locality and attacking a definite object, exactly repeating the act.

Witmer[3] records observations on an unusually talented chimpanzee, whose behavior was marked by rapid imitation and ready response to verbal directions; also[4] on the quick solving of latch problems by a Macacus monkey.

M. F. W.

[1] G. P. Zeliony, 'Ueber die Reaktion der Katze auf Tonreize,' *Zent. f. Physiol.*, Bd. 23, S. 762, 1910.

[2] M. E. Haggerty, 'Imitation in Monkeys,' *Jour. Comp. Neurol. and Psychol.*, vol. 19, p. 337, 1909.

[3] L. Witmer, 'A Monkey with a Mind,' *Psych. Clinic*, vol. 3, p. 179, 1909.

[4] Ibid., 'Intelligent Imitation and Curiosity in a Monkey,' *Psych. Clinic*, vol. 3, p. 225, 1900.